**Atmospheric
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Comment

Interactive comment on "Global trends in visibility: implications for dust sources" *by* N. M. Mahowald et al.

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Received and published: 9 April 2007

***** General Comments *****

This article represents an ambitious attempt to relate visibility observations to measurements related to the mobilization of dust aerosols. One goal is to distinguish natural sources of dust from sources created by human activity. This is an important problem, and shares a theme with previous work by the first author. I am recommending publication after revision because the article represents one of the few global analyses of this data set. However, there are substantial uncertainties that need to be addressed.

There seem to be four main conclusions. First, surface extinction, where light attenuation is measured within air at the surface, is a better indicator of local aerosol sources

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than column optical thickness, where attenuation is measured vertically across the entire column. This is because column extinction includes aerosols high overhead that may be transported from faraway sources. Visibility, which measures light attenuation along a horizontal path is related to surface extinction, because attenuation occurs within air at the surface. The article would benefit by making these points more explicit and prominent. I couldn't find a definition of surface extinction in the article. As a reader with limited experience in radiative transfer, I could only infer through context the distinction with optical thickness, so that one of the interesting points of the article was initially lost on me.

The second conclusion is that temporal variations of visibility can be related to rainfall over Africa and surface wind speed over East Asia, but otherwise exhibit little correlation with other variables such as cultivation and grazing or climate indices related to El Nino and the North Atlantic Oscillation. I have some technical comments below, but these conclusions are consistent with previous studies (e.g. Prospero and Lamb 2003 for the Sahel; Sun et al JGR 2001 for East Asia), and seem robust.

Thirdly, the authors conclude that spatial variations of visibility are better correlated to cultivation than other indicators of dust sources, such as topographic lows. They then conclude that either are 'equally good at inferring...dust surface fluxes' (p.22). I have reservations about this comparison. As the authors note (p.2), cultivation is often colocated with natural sources of dust, so that using spatial correlations to distinguish the influence of each upon variations in visibility is ambiguous. In addition, the network of visibility measurements, while extensive, does not include large parts of the Sahara that act as dust sources. (see Figure 5: I believe the gray shading indicates something related to the TOMS AAI, which is intended as a proxy for natural sources, although this is not stated in the caption nor could I find the words 'gray' or 'shading' in the text.) As the authors note, surface extinction and visibility are attractive measurements because they are more sensitive than column extinction to local aerosol sources. However, their corresponding drawback is that sources far from the observing network will be difficult

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to detect. Visibility measurements are taken in locations of human settlement, which are generally supported by nearby agriculture. Thus, the visibility measurements are positioned to be more sensitive to cultivated sources of dust, and will exaggerate their importance if natural sources are comparatively remote. That the usual metrics of natural sources (e.g. topographic lows) correlate with TOMS AAI variability (Prospero et al. 2002) but show no significant correlation with visibility raises the question whether the density of the visibility network is sufficient. Another issue is the influence of other aerosol species on the visibility. While the correlation is limited to sites where dust contributes at least half of the annual average surface extinction, this allows a substantial contribution from other aerosol species. Anthropogenic sulfates originating from power plants or black carbon from inefficient combustion (e.g.), will reduce visibility and correlate with nearby cultivation. While I wouldn't rule out an important contribution to dust from cultivated sources, the visibility measurements presented here seem insufficient to contradict the 'hypothesis that dry lake beds are dust sources' (p.22). I also don't see any quantitative criteria that can be used to argue that the TOMS AAI and visibility network are 'equally good at inferring...dust fluxes' (p.22). It seems more defensible to argue that visibility measurements indicate the importance of local sources in the vicinity of cultivated areas. Since the significance of cultivated sources hasn't been established definitively in the literature, it would be useful if it could be shown that visibility is reduced in regions far from natural dust sources and where other aerosol species are negligible.

Finally, the authors conclude that while spatial variability of dust sources is related to cultivation, temporal variations in the last few decades are related to climate variables like rain in Africa and wind speed in East Asia. Given large changes in cultivation over this period related to the near-doubling of world population, it surprises me that cultivation fails to leave an imprint on temporal trends if it is such an apparent source of spatial variations in dust. One problem may be the low temporal resolution of each land use time series, which might obscure its correlation with higher resolution data sets. That the expansion of cultivated areas to match the growing population has little

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influence on visibility seems to highlight the uncertainty in both data sets.

In summary, I think there is too much uncertainty to assess the relative importance of natural and cultivated sources. I would recommend that the authors give more emphasis to using to visibility observations to show the existence of cultivated sources. The article represents a substantial analysis of the visibility measurements, a potentially important source of information about dust that has received little attention, with the exception of Engelstaedter et al 2002 and various Chinese authors. The authors deserve credit for trying to find a common picture among such a heterogeneous set of data. I am including my email address (rmiller@giss.nasa.gov) in case the authors have any questions about my review, or want to correct possible misunderstandings on my part, or wish to consider the amount of dust in my office as a potentially large and currently overlooked anthropogenic source.

***** Specific Comments *****

p.1 (abstract): After the second sentence promising 'to assess the anthropogenic impact on long term trends in desert dust emissions', insert a sentence briefly describing how you are going to do this.

p.1 (abstract): replace '˜0.47' with '0.47'?

p.3 replace 'one long time series dataset' with 'one long time series'.

p.3 delete 'as well as the potential for other problems' or list specific problems?

p.3 'we try to derive a representative proxy from a global data set *to correlate with visibility observations*'?

p.3 'Very little is know*n*'

p.4 (citation)?

p.5 In order to evaluate the visibility measurement...' Given that you ultimately conclude that visibility is better suited than column extinction to measure aerosol surface

concentration, it seems paradoxical to use AERONET AOD to reassure yourself that visibility measurements are sensitive to aerosol variations. A poor correlation might simply result from the presence of lots of far-traveled aerosols above the boundary layer, and not the insensitivity of visibility to surface aerosols.

p.5 citation to Mbourou et al 1997: Sharon Nicholson (a coauhtor on this paper) cites it as N'Tchayi Mbourou et al 1997.

p.5 'surface extinction value through Koschmeider's formula.' Please define surface extinction. If I'm not mistaken, it represents light extinction within air at the surface and thus provides a measure of surface concentration of absorbers, including aerosols, water vapor and clouds. Please also relate surface extinction to measurements of visibility. You might also note that surface extinction is less prone than column optical thickness to contamination by aerosols passing overhead from remote sources. Readers like me, who aren't radiatively adept, might otherwise miss the significance of your first major conclusion.

p.5 '...here we compare them (TOMS) against the AERONET optical depths.' What is the goal for doing this? Note that AERONET AOT measures extinction by all aerosols including sulfates, whereas the TOMS AAI is sensitive mainly to absorbing aerosols, so the two AOT may differ even if they have the same sensitivity to dust.

p.6 '(PDSI) incorporates antecedent precipitation...' Please provide a citation so that the reader can find the exact formula if necessary.

p.7 'rank correlations, for which we know the distribution...' I thought rank correlations were attractive because you didn't have to know the distribution.

p.7 'we arbitrarily choose >25%' Shouldn't this depend upon the number of independent observations and the number of variables?

p.7 'replaceable' If explained variance is reduced by more than 25% when a variable is omitted, shouldn't it be 'irreplaceable'?

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p.7 MATCH: what time period corresponds to the model simulations?

p.7: Given that MATCH/DEAD is used to identify dusty regions, describe how sources of dust and other aerosol species are prescribed in the model.

p.8 '...elucidates the theoretical relationship between dust sources and extinction' Is there a theoretical relationship? Alternatively, I thought the value of the model was that you could relate sources and extinction in the absence of data gaps and contimination by other aerosol species.

p.8 correlation of AOD to surface extinction (calculated from visibility): did you remove the seasonal cycle from each time series prior to computing the correlation?

p.8 'we correlate the values collectively?' Did you correlate the station-averaged variables or did you average the individual correlations at each station?

p.9 Maryland Science Center vs GSFC: AOD at both these locations is predominately influenced by urban pollution such as sulfates and carbonaceous aerosols with sources broadly distributed across the eastern seaboard. Thus, it probably isn't a uniformly good indicator of the effect of small-scale aerosol variations (e.g. urban pollution within largely rural areas).

p.9 'Note that if we perform the correlation over all the *AERONET* stations...'?

p.10 'best correlations...3-7 km' Are the correlations at 2 and 9 km significantly different to warrant exclusion from this range?

p.10 correlation of TOMS and AERONET: again, is the mean seasonal cycle included in this correlation?

p.10-11 linearly proportional to concentration of *aerosols, water vapor, and cloud water*'?

p.11 replace 'Thus, we are using...' with 'Nonetheless, we are using...'?

p.11 'For this section, the results are...' Could you be specific?

p.11 'modelled maximum in dust AOD or 'surface concentration...' Figure 2 is labelled with 'surface extinction' rather than 'surface concentration'. See also 'The surface concentrations appear...'

p.12 '(even at a limited number of stations)' I suggest deleting this phrase because it is qualitative and the issue is discussed in the next paragraph.

p.12 replace 'much better job' with 'better job' unless you can quantitatively defend 'much'?

p.12 'Our analysis suggests that...' This seems like a comparison of apples and oranges. Your benchmark for quality is agreement with AERONET AOD. Yet, TOMS and visibility are expected to disagree with AERONET for different reasons: TOMS should disagree because it is more sensitive to absorbing than reflecting aerosols, and the visibility measurements should disagree because they measure horizontal extinction rather than the column extinction related to AOD.

Section 4: A lot of discussion is devoted to large fluctuations in spatial averages early in each record when there are significantly fewer observations and thus greater uncertainty in the spatial averages. I suggest adding error bars to each time series in Figure 6 (and similar figures) so that the reader can decide which fluctuations are robust. One point to note is that not all observations used in the spatial average are independent due to geographic proximity, so that the number of independent observing sites used to estimate the confidence interval will be less than the total.

Figure 5: I probably overlooked it, but do you explain the gray contours? I'm guessing that it is related to TOMS AI, but this is not explained in the caption.

p.13 'The Bodele basin does not appear to be the largest source of dust...' I have several reservations about this statement: i) other aerosol species are allowed to contribute up to 50% of the surface extinction so that there is an imperfect (and spatially

variable) relation between dust and visibility, and ii) you only have one station in the vicinity of the Bodele depression according to Figure 5.

p.13 'associated with Sahel drought...' I don't see large decadal variations in precipitation in Figure 6 that are associated with the Sahel drought (e.g. Prospero and Lamb Sci 2003), and I wonder whether you might find a stronger relation with visibility limiting the spatial average of precipitation to a subset of the African continent showing a high correlation between visibility and precipitation? Sharon Nicholson has noted that the decrease in Sahel rainfall is often associated with an increase in rainfall to the south along the Guinea coast, and this regional compensation may reduce the apparent variability in rainfall over the dust source regions (Nicholson Rev Geophy 2000).

p.14 'We focus next on correlations...' Are these correlations computed from annual averages? If monthly averages are used, is the seasonal cycle included in each time series? It seems more appropriate to compute the correlation with monthly averages after subtracting the seasonal cycle if you are interested in relations over interannual and decadal time scales. Otherwise, a high correlation may simply indicate that the seasonal cycle of the two variables is in phase, and not that there is a longer term relation.

p.14 'and boxes indicate that the correlations only exist between that variable and the visibility derived variable.' I don't understand this.

p.15 'positive correlation between EXT and cropland...' The cropland and grazing time series have only about 10 or so independent values, given the low temporal resolution of the data sets. Thus, it is much easier to get a high correlation by chance compared to a data set like visibility where values from successive years are probably independent. Was this reduction in the number of degrees of freedom accounted for when computing the significance of correlations with cropland and grazing?

p.15 'China...in the 1940's' According to Figure 13, there is no visibility data for this region in the 1940's.

p.16 'This makes some sense...' Is it possible that the positive correlation is because higher humidity during wet years increases haze and reduces visibility?

P.18: When computing statistical significance of correlations, did you account for the possible dependence of certain stations? If the stations are not all independent, then the threshold for significant correlation will increase.

p.18 'These results suggest that cultivation is the best determinant of spatial variability of dustiness, ...not whether there are topographic lows nearby.' That the correlation between visibility and cultivation decreases when the cultivation resolution is increased (so that the visibility stations and cultivation are no longer precisely colocated) suggests that the visibility data are strongly influenced by local sources, which will favor cultivation compared to natural sources.

p.22 'The hypothesis that dry lake beds are dust sources is not supported...' I'm skeptical of this conclusion, because the visibility measurements seem disproportionately sensitive to local sources of aerosols.

Figure 1: the AERONET locations are indistinct: use a higher contrast color like white and a thicker line?

Figure 6 (and similar figures): the scale for cultivation should be reduced so that temporal changes are more apparent?

Figures 7, 11, 13, 15, 17, 19 and 21 are missing minus signs on the color bar.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3013, 2007.

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